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ABSTRACT

In a public school setting administrators are frequently under local pressure to make a new project service available to all eligible children. However, comparable control groups for project evaluation are often absent, and although random assignment to treatment groups remains the most systematic method of providing controls, this is not often possible in the realities of operating a big-city school system. Several experimental designs, including time-series studies, are suggested as a means of overcoming this methodological problem in project evaluation. (DG)

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A SEARCH FOR ALTERNATIVES TO RANDOM ASSIGNMENT TO TREATMENT GROUPS[1]

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One of the essential elements of a good experimental design is random assignment of units to treatment groups. Randomization according to Campbell, "renders implausible innumerable rival explanations of observed changes by cutting the lawful relationships which in the natural setting would determine which person gets which treatment" (Trismen, 1965). Such a model is represented by Design No. 1:

DESIGN NO. 1:

R	O_1	X	O_2
R	O_1		O_2

These symbols indicate random treatment assignment of units to two groups, one of which receives the treatment.

In a public school setting, randomization is not that easily attained. Administrators responsible for implementing project operations are generally under local pressure to make service available to everybody who meets eligibility criteria. To convince administrators to do otherwise in the face of such

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pressure is impossible. But evaluate we must, and some compromise between the ideal and the possible has to be made. This presentation attempts to discuss what we have been doing because Design No. 1 is not practical in our setting, using approaches based on the logic of Campbell and Stanley (1967).

The simplest standard design is 2:

DESIGN NO. 2: O_1 X O_2

where observations prior to and after application of Treatment X to a group was made. Change scores or post scores of Project children may be compared with that of a designated group which may or may not be comparable with the experimental group. For example, pre-post change of Project children on a Stanford-Binet test may be compared with normal change reflected in the standard error of the instrument, which is utilized by test publishers for normative purposes. The designated comparison group in such a case is the standardization group. This design may enable us to know whether real change had occurred, but is it possible to infer that Treatment X was the cause? The Project evaluated may be altering schedules or producing changes in participants and staff, which could be the true cause of change. The presence of systematic differences between the local experimental and the national normative group is one of the most plausible rival hypotheses we have to contend with. Some of the tests in the market for example, are normed on groups that are not truly representative of the population we are comparing it with.

In the absence of a more appropriate alternative we sometimes utilized Design No. 2 while looking for ways of improving it. Take for example, the evaluation of Title I Child Development Project which is referred to as Head Start in most places. As with most Title I projects children are enrolled in Child Development classes as long as there is available classroom space -- a practice which probably prevails in most public school settings. Ideally, random treatment assignment could be achieved by denying services to every nth child in order to achieve Design No. 1. Such an ideal solution is the practice in most university-sponsored preschool programs where research-development and operations go hand-in-hand. In a public school setting, where there is a marked break between research-development on one side, and project operations on the other, this is simply not done. Some compromise had to be reached in order to have a control group. We decided to use children on the waiting list to serve as quasi-controls, only to discover later that this would not work out. We learned then that children on the waiting list were accepted at any time during the school year whenever a vacancy occurred. So we are back to where we were: no controls and a self-selected experimental group.

To resolve the problem of no controls, we planned a two-phase evaluation. Phase I utilized Design No. 2, which provided data for required state and federal reports. Phase II involved the use of an extended Design No. 2, which could take the form of either Design 3 or 4:

DESIGN NO. 3:

X_1	O_1	X_2	O_2
	O_1	X_2	O_2

DESIGN NO. 4:

X_1	O_1	X_2	O_2
	O_1	X_2	O_2
X_1	O_1		O_2
	O_1		O_2

X_1 refers to the experiences in Child Development Project.

X_2 refers to the experiences in different types of Kindergarten programs.

These designs made possible not only the assessment of preschool experiences a year later at entry and at the end of the kindergarten year, but also the degree to which such experiences are affected by different kindergarten programs such as the expensive and intensive Kindergarten Follow-Through, or state-funded Kindergarten Enrichment Project.

One of the new features about Designs 3 and 4 is that it involved the use of local control groups, which were missing in Design 2. A more simple illustration of this model where a local control group is introduced is represented by this standard design, Design No. 5:

DESIGN NO. 5:

O_1	X	O_2
O_1		O_2

Instead of a designated standardization group for comparing the local experimental group with, now a local control group is introduced, representing an improvement over Design 2. One of the obvious advantages of Design No. 5 is the elimination

of systematic differences between the treatment and the standardization groups. You would notice, however, that the randomization procedure referred to in Design No. 1 is missing.

In working with this design, we have resorted to statistical controls such as the Analysis of Covariance, to equate or adjust for any systematic variations between these treatment groups. However, "adjustment" has limitations in terms of imperfect measurement of criterion and covariate, and the infinite number of dimensions on which the groups could covary.

The impracticability of Design No. 1 and the limitations of standard Designs 2 and 5, led us to consider the issues of cross-sectional and longitudinal studies. We decided on time-series studies, extending in time designs presented earlier. The essence of time-series designs is periodic measurement on some group or subject, before or after the introduction of Treatment or Treatments X into these measurements. This is illustrated by Design No. 6:

DESIGN No. 6;

O_1	X	O_2	O_3	$O_4 \ldots O_j$
O_1		O_2	O_3	$O_4 \ldots O_j$

The 1968 - 1969 evaluation of state-funded Kindergarten Enrichment Project represents an application of this model. This Project has identifiable unique components designed to provide continuity of enriching educational experiences initiated at the Title I Child Development preschool project. This Project operated at 31 Title I target schools which has been designated on the basis of high percent of families on public assistance, commonly referred to as poverty index.

Schools with high poverty indices are assigned target status, which means eligibility for federal-and state-funded special projects. Poverty indices of the Title I target schools ranged from 56% to 22%, while those of Title I non-target schools ranged from 37% to 17%. Inclusion of these schools with lower poverty indices among the target schools was made at the expense of other schools with higher indices to accommodate racial balance of target status designation.

The ten experimental schools selected from Title I target schools reported poverty indices ranging from 39% to 29%. The selection of these experimental schools was based primarily on the comparability of their poverty indices with those of the control schools selected from the Title I non-target schools. Poverty indices of these control schools ranged from 35% to 23%. After designating the experimental and control schools, experimental and control children were randomly sampled from these schools.

Thus, designation of these treatment groups was based on an explicit criterion of poverty index. The randomization procedure was involved only in sampling subjects after treatment groups were selected. Questions of comparability of groups, and internal validity of findings could easily be invoked to the extent that random assignment of treatment groups was not made. However, the longitudinal focus of this evaluation allows us to work around the issue of randomization.

The study represents a simultaneous evaluation of two projects, Child Development and Kindergarten Enrichment, using either Design 7 or 8:

DESIGN NO. 7:

X_1	O_1	X_2	O_2	O_3	$O_4 \ldots O_j$
	O_1	X_2	O_2	O_3	$O_4 \ldots O_j$

DESIGN NO. 8:

X_1	O_1	X_2	O_2	O_3	$O_4 \ldots O_j$
	O_1	X_2	O_2	O_3	$O_4 \ldots O_j$
X_1	O_1		O_2	O_3	$O_4 \ldots O_j$
	O_1		O_2	O_3	$O_4 \ldots O_j$

Design No. 8 was used because it allowed for the assessment of main effects which could be attributed to Kindergarten Enrichment and Child Development experiences, and interaction effects of these two factors. Continuing assessment of these children, now in first grade are kept up in terms of yearly administration of the same test instruments, follow-up of attendance data, mobility rate or transfers to different schools, and records of participation in different projects.

One of the problems in time-series studies is the occurrence of "multiple treatment interferences" which appear to be a great source of compounding and contaminating errors. This problem became very real to us when we initiated this year to follow-up the 1965 Child Development Project participants on attendance and test-data accumulated over five years from the city-wide testing program. Older siblings of participants were selected to serve as controls. Evaluation Design 9 was used:

DESIGN NO. 9:

	X_1	O_1	O_2 $O_3 \ldots O_j$
O_1	O_2	$O_3 \ldots O_j$	

As we traced records of the experimental children who are presently at fourth grade back to kindergarten, we noted their exposure to multiple X treatments, as a result of increasing use of federal funds in the schools. Thus, what had started as an assessment of the sustained value of one project five years later, was expanded to include other projects and their obvious interactions. Assuming that school records have some degree of reliability which could give us the list of X treatments for any given child we anticipate a problem which could affect the internal validity of our results. We are faced with the problem of identifying which of the X treatment or treatments did in fact make the real difference in the children's performance. From the point of external validity, the results will be unique to the population, subject of course to replication of this kind of assessment.

Time-series studies are no doubt cumbersome and messy. It might even be unmanageable in the absence of a computerized data bank on the pupils. Its appropriateness, however, in a school setting where attendance and other school records are kept, and annual achievement and aptitude tests constitute such a natural part of the school environment, cannot be denied.

The extended and modified designs we have presented are part of our local efforts to come up with workable evaluation models in a natural setting where "control" is an elusive concept. The luxury of providing a "control" to natural conditions is impossible for being natural and real is antiethical to "control" (Guba, 1969). If evaluation designs are to be workable in our present setting, we need to build upon and modify our present knowledge of the classical experimental research paradigm.

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